ASTRONOMICAL PHENOMENA FOR THE WEEK 1886 NOVEMBER 28—DECEMBER 4

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 28

Sun rises, 7h. 42m.; souths, 11h. 48m. 10·4s.; sets, 15h. 55m.; decl. on meridian, 21° 21′ S.: Sidereal Time at Sunset, 20h. 25m.

Moon (three days after New) rises, 9h. 52m.; souths, 14h. 15m.; sets, 18h. 39m.; decl. on meridian, 19° 7′ S.

Planet	Rises	Souths	Sets	Decl. on meridian
	h. m.	h. m.	h. m.	° ′
Mercury	8 40	12 35	16 30	23 11 S.
Venus	7 33	11 43	15 53	20 51 S.
Mars	10 27	14 14	18 1	24 15 S.
Jupiter	3 55	9 12	14 29	9 14 S.
Saturn	19 5*	3 7	11 9	21 25 N.

* Indicates that the rising is that of the preceding evening.

Occultation of Star by the Moon (visible at Greenwich)

Dec.	Star	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image
			h. m.	h. m.	° °
3	h^1 Aquarii	5½	17 8	18 30	116 286

Dec.	h.	
3	5	Venus in superior conjunction with the Sun.
3	12	Mercury in inferior conjunction with the Sun.
4	4	Mercury at least distance from the Sun.

Variable Stars

Star	R.A.	Decl.			
	h. m.	° ′		h. m.	
U Cephei	0 52·2	81 16 N.	Nov. 28,	2 27	*m*
			Dec. 3,	2 6	*m*
Algol	3 0·8	40 31 N.	,, 1,	5 38	*m*
			,, 4,	2 27	*m*
ζ Geminorum	6 57·4	20 44 N.	Nov. 29,	0 0	*m*
			Dec. 4,	0 0	*M*
U Monocerotis	7 25·4	9 32 S.	Nov. 28,		*m*
S Cancri	8 37·4	19 27 N.	,, 28,	3 9	*m*
T Ursæ Majoris	12 31·2	60 7 N.	,, 29,		*M*
S Virginis	13 27·1	6 37 S.	,, 30,		*M*
β Lyræ	18 45·9	33 14 N.	,, 28,	21 30	*m*
			Dec. 2,	5 0	*m*
R Lyræ	18 51·9	43 48 N.	Nov. 28,		*M*
η Aquilæ	19 46·7	0 43 N.	Dec. 1,	2 30	*M*
δ Cephei	22 24·9	57 50 N.	Nov. 30,	2 0	*m*

M signifies maximum; *m* minimum.

Meteor Showers

The chief shower of the week is that of the *Taurids;* R.A. 60°, Decl. 49° N. Other radiants active at this time are as follows:—Near η Persei, R.A. 44°, Decl. 56° N., slow, faint meteors; near α Canum Venaticorum, R.A. 194°, Decl. 43° N., very swift, streak-leaving meteors; from Leo Minor, R.A. 155°, Decl. 36° N.; from near η Ursæ Majoris, R.A. 208°, Decl. 43° N. Fireball dates, November 29 and December 2.

TEN YEARS' PROGRESS IN ASTRONOMY[1]

II.

The Solar Spectrum.—In 1877 Dr. Henry Draper, of New York, by a series of most laborious, time-consuming, and expensive researches, discovered the presence of oxygen in the sun, evidenced in his photographs, not by fine dark lines, as in the case of elements previously recognised, but by bright, hazy bands. It is difficult to assign any reason why this gas should behave so peculiarly and so differently from others, and for this reason many high authorities are indisposed to accept the discovery. But the evidence of the photographs seems fairly to outweigh any such purely negative theoretical objections.

Other advances have been made in the study of the spectrum, due mainly to the great improvements in spectroscopic apparatus. Until recently it has not been easy to decide with certainty as to some lines in the spectrum whether they were of solar or telluric origin; the great bands known as A and B, for instance. It was only in 1883 that the Russian, Egoroff, succeeded in proving that these are produced by the oxygen in the earth's atmosphere. In his experiments, on a scale previously unknown, the light was transmitted through tubes more than 60 feet in length, closed at the end with transparent plates, and filled with condensed gas.

It was quite early pointed out that the sun's rotation ought to produce a shift in the position of lines in the spectrum according as the light is derived from the advancing or receding edge of the solar disk, and Zollner thought he could perceive it. The earliest *measures*, however, were, I believe, those obtained independently by Vogel and the writer in 1876. In the great bisulphide of carbon spectroscope of Thollon the displacement becomes easy of observation; and very recently Cornu, by taking advantage of it, and by an extremely ingenious arrangement for making a small image of the sun to oscillate across the spectroscope slit two or three times a second, has been able to discriminate at a glance between the telluric and solar lines; the former stand firm and fast, while the latter seem to wave back and forth.

In this connection also should be mentioned the great map of the solar spectrum, for which Thollon received the Lalande Prize of the French Academy of Sciences last January, and the still more accurate and important map photographed by Prof. Rowland, by means of his wonderful diffraction-gratings, and now in course of publication. Nor would it be just either to omit the earlier and less accurate maps of Fievez and Vogel, which, when published, were as far in advance of anything before them as they are behind the new ones; nor the maps just made by Prof. Smyth, of Edinburgh.

It was in connection with the construction of such a map by Mr. Lockyer, that he was led to his theory of the compound nature of the so-called chemical elements, partly as a result of his comparisons of the spectra of different substances with the solar spectrum, and partly in consequence of considerations drawn from certain phenomena observed in the solar and stellar spectra themselves. His first paper on the subject was read late in 1878. This "working hypothesis," as its author calls it, has met with much discussion, favourable and unfavourable. It unquestionably removes many difficulties and explains many puzzling phenomena; at the same time there are very serious objections to it, and some of the arguments upon which Mr. Lockyer originally laid much stress have turned out unsound. For instance, he made a great point of the fact that, after all precautions are taken to remove impurities, several elementary substances show in their spectra common lines—"basic lines" he called them—indicating, as he thought, a common component. He found in the solar spectrum about seventy of these "basic lines." Now, under the high dispersion of our newer spectroscopes, these lines, which were single to his instruments, almost without exception dissolve into pairs and triplets, and withdraw their support from his theory.

I suppose that at present the weight of scientific opinion is against him; but, for one, I do not believe his battle is lost. In view of the law of Dulong and Petit, which establishes a relation between the atomic weight and specific heat of bodies, it seems to be pretty certain that *hydrogen* cannot be the elementary "*urstoff*" out of which all other elements are made by building up, as he at first seemed disposed to maintain; this element stands apparently on no different footing from the rest. But I see no reason why the elements, as we know them, may not constitute one *class* of bodies by themselves, all built up out of some as yet more elemental substance or substances. The "periodic law" of Mendelejeff suggests such a relation. And our received theories so stumble, hesitate, and falter in their account of many of the simplest phenomena of the solar and stellar atmospheres, that a strong presumption still remains in favour of the new hypothesis. I am not prepared to accept it yet; but certainly not to reject it.

The Chromosphere.—The study of the chromosphere and prominences has been kept up, very systematically and statistically, by Tacchini in Italy, and with less continuity, but still assiduously, by several other observers. I do not know, however, that any new results of much importance have been arrived at. The list of bright lines visible in their spectra has been a good deal enlarged; and Trouvelot thinks he has observed *dark* prominences—objective forms that show, black but active, upon the background of bright scarlet hydrogen in the surrounding chromospheric clouds. It may be that he is right; but, so far as I can learn, no other observer of the solar atmosphere has seen anything similar. I certainly have not myself. And I think some of his published observations of velocities of two or three thousand miles a second in the motions of the prominences, as evidenced by the displacement of lines in the spectrum, are still more questionable.

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[1] "Ten Years' Progress in Astronomy, 1876-86," by Prof. C. A. Young. Read May 17, 1886, before the New York Academy of Sciences. Continued from p. 69.

In two or three cases, prominences have been observed since 1876 considerably higher than any known previously. In October 1878 I myself observed one which attained an elevation of nearly 400,000 miles (13½').

Eclipses and the Corona.—The sun's corona has been, perhaps, more earnestly studied than anything else about the central luminary, especially during the four eclipses which have occurred since 1876. At the eclipse of 1878, in the midst of an epoch of sunspot quiescence, the corona was found less brilliant than ordinary, and especially deficient in the unknown gas that produces the so-called 1474 line—the line which characterises the spectrum of the corona, and first demonstrated conclusively its solar origin in 1869. But while the corona at this time was less brilliant than it had been formerly, it was far more extensive. At least it seemed so; for, at Pike's Peak and Creston, Langley and Newcomb were able to follow its streamers to a distance of 6° from the sun. It is possible, however, that this extension was only due to the superior transparency of the mountain air.

The Egyptian eclipse of 1882 gave us some interesting results respecting the spectrum of the prominences and the corona. It appears that the light of the corona is especially rich in the ultra-violet, and in the photographs of the spectrum a number of bands are found which have been interpreted, with questionable correctness I think, as indicating the presence of carbon. The eclipse of 1883 was observed in the Pacific Ocean by French and American parties, but, I think, added very little real information. Prof. Hastings made an observation which he believed to establish a peculiar theory proposed by himself, viz. that the corona is merely a diffraction effect produced by the moon's limb, and depending on the non-continuity of phase in long stretches of light-vibrations. With a peculiar apparatus prepared expressly for the purpose, he found that at any moment the 1474 line was visible to a much greater distance from the sun on the side least deeply covered by the moon than on the other: as unquestionably would happen if his theory were correct. But the same thing would result from the mere diffusion of light by the air; and, notwithstanding his protests, the French observers who were at the same place, and nearly all others who have discussed the observations, think that this was the true explanation of what he saw. So far as I know, the discussion of the subject which has resulted from his publication has only strengthened the older view—that the corona is a true solar appendage; an intensely luminous but excessively attenuated cloud of mingled gas and fog and dust surrounding the sun, formed and shaped by solar forces.

The diffraction theory has one advantage—that it relieves us from stretching our conceptions as to the possible attenuation of matter to the extent necessary in order to account for the fact that a comet, itself mostly a mere airy nothing, experiences no perceptible retardation in passing through the coronal regions. There can be no question that this has happened several times: the last instance having been the great comet of 1882. But on careful consideration it will be found, I think, that our conceptions will bear the stretching without involving the least absurdity; a single molecule to the cubic foot would answer every necessary condition of the luminous phenomenon observed. And all the rifts and streamers, and all the radiating structure and curved details of form, cry out against the diffraction hypothesis.

The observations of the eclipse of 1885 (observed only by a few amateurs in New Zealand) have not proved important.

At present the most interesting debate upon the subject centres around the attempt of Mr. Huggins (first in 1883) to obtain photographs of the corona in full sunlight. He succeeded in getting a number of plates showing around the sun certain faint and elusive halo forms which certainly look very coronal. Plans were made and have been carried out, for using a similar apparatus on the Riffelberg, in Switzerland, and at the Cape of Good Hope. But so far nothing has been obtained much in advance of Mr. Huggins's own first results. Since September 1883, until very recently, the air has been full, as every one knows, of a fine haze, probably dust and vapour from Krakatão, which has greatly interfered with all such operations. It is now fast clearing away, and I for one am somewhat sanguine that a much greater success will be reached next winter at the Cape, and perhaps even in England during the coming summer.

Just about the same time that Huggins was photographing in England, Prof. Wright was experimenting in New Haven in a different way: isolating the blue and ultra-violet rays by the use of coloured media, stopping out the sun's disk, and receiving the image of the coronal regions on a fluorescent screen. He also had obtained what he believed, and still believes, to be a real image of the corona, when the aërial haze intervened to put an end to all such operations; for of course it is evident that whether one operates by this method or by photography, success is possible only under conditions of unusual atmospheric transparency and purity.

I suppose at present the predominant feeling among astronomers is that the case is hopeless, and that Huggins and Wright are mistaken. It may be so. But my own impression is that they are probably correct; although, of course, the matter is still in doubt.

Inferior Planets.—Leaving now the sun, and passing to the planetary system, we come first to the subject of intra-Mercurial planets.

The general opinion among astronomers (in which I fully concur) is that the question has been now fairly decided in the negative, *i.e.* it is practically certain that within the orbit of Mercury there is no planet of a diameter as large as five hundred miles, probably not one hundred. If such a one existed, it could not have failed to be discovered by the wide-angled photographs taken at the eclipses of 1882 and 1883, to say nothing of the visual observations. Of course, it is well known that at the eclipse of 1878 Prof. Watson supposed he had discovered two such bodies, and his extensive experience and his high authority led, for a time, to a pretty general acceptance of his conclusion. I notice that Dr. Ball, even very lately, in his "Story of the Heavens," is still disposed to credit the discovery. But Dr. Peters, by a masterly discussion of the circumstances of the observations themselves, and a comparison with the star maps, has shown that it is almost certain that Watson really saw only the two stars θ and ζ Cancri. In the same paper also, Peters examined all the observations of small, dark spots crossing the sun's disk which, up to that date (1879), had been made by Leverrier and others the ground for their belief in "Vulcan"; and he shows that they really afford no sufficient ground for the conclusion. As to Mr. Swift's supposed observation of two objects with large disks "both pointing to the sun," they certainly were not the two seen by Watson, while they were in the region covered by Watson and several other observers. What the precise nature of the mistake or illusion may have been it is perhaps not now possible to discover, but I think no one, unless perhaps Mr. Swift himself, now considers the observation important.

While, however, the question of a "Vulcan" is now pretty definitely settled, it is not at all impossible, or even improbable, that there may be intra-Mercurial asteroids, and that some of them may be picked up as little stars of the sixth magnitude or smaller, by the photographers at the eclipse of next August, or in 1887. The sensitiveness of our present photographic plate is now many times greater than it was even in 1882.

As to the planet Mercury, there is very little to report. It "transited" the sun in May 1878, and again in November 1881, and during the transits numerous measures were made of its diameter, giving results substantially in accord with the older values. I have already alluded, in connection with the earth's rotation, to Newcomb's investigation of former transits of this planet as establishing the sensible uniformity of the earth's rotation.

The planet Venus, by her transit in 1882, has attracted much attention, and much interest is felt as to the final outcome of the whole enormous mass of data, photographic and visual. Just how long we shall have to wait for the publication seems still uncertain. I have already said, however, that probably these transits will never again be considered as important as hitherto.

The most important physical observations upon the planet during the decade seem to be those of Langley, who, during the transit of 1882, observed a peculiar, and so far unexplained, illumination of one point on the edge of the planet's disk, and those of Trouvelot and Denning, who have observed and figured certain surface-markings of the planet. I think I may fairly mention also our Princeton observation of the spectrum of the planet's atmosphere during the transit, and our confirmation of Gruithuisen's old observation of a white cap (likely enough an ice-cap), at the edge of the planet's disk—probably marking the planet's pole, and showing that the planet's equator has no such anomalous inclination of 50° or 60°, as stated in some of the current text-books. This cap has also been observed by Trouvelot and Denning. But this lovely planet is most refractory and unsatisfactory as a telescopic object, apparently enveloped in dense clouds which mostly hide the real surface of the globe, and mock us with a meaningless glare.

We mention in passing, but without indorsement, the speculations of Houzeau, who has attempted to account for some of the older observations of a satellite to Venus, by supposing another smaller sister planet, "Neith," circling around the sun in an orbit a little larger than that of Venus, and from time to time coming into conjunction with it. But the theory is certainly untenable; a planet large enough to show phases, as the hypothetical satellite is said to have done, in the feeble telescopes with which many of the observations were made 100 years ago or more, would be easily visible to the *naked eye even.* There can be little doubt that all the Venus satellites so far observed are simply *ghosts* due to reflections between the lenses of the telescope, or between the cornea of the eye and the eye lens.

Mars.—But while Venus has gained no moons during the past ten years, Mars has acquired two, and they are both native Americans. There is no need to recount the faithful work of Prof. Hall with the then new great telescope at Washington, and its brilliant result; brilliant in a scientific sense, that is, for, regarded as luminaries, it must be admitted that the Martial satellites, in spite of their formidable names of Phobos and Deimos, do not amount to much. Under the best of circumstances, they are too faint to be seen by any but keen eyes at the end of great telescopes. Small as they are, however, the little creatures punctually pursue the orbits which Hall has computed for them, and, when the planet came to its opposition a few weeks ago, they were found just in their predicted places. They are interesting, too, from the light they throw upon the genesis and evolution of the planetary system, almost compelling the belief that they have come *gradually* into their present relation to the planet. The inner one, Phobos, revolves around the primary in 7h. 39m., which is less than one-third of the planet's day. The theory of "tidal evolution," proposed by Prof. G. H. Darwin in 1878-80, as the result of his investigations upon the necessary mechanical consequence of the tidal reactions between the earth, sun, and moon, will account for Phobos, and I know nothing else that will; though, of course, it would be rash to assert that no other account can ever be given.

Much attention has also been paid to the study of the planet's surface. In 1876 we were already in possession of three elaborate maps, by Proctor, Kaiser, and Terby, agreeing in the main as to all the characteristic formations. In 1877, Schiaparelli, of Milan, detected, or thought he did, on the planet's surface, a numerous system of "canals"—long, straight channels, some of them more than 1000 miles in length, with a pretty uniform width of fifty or sixty miles; and from his observations he constructed a new map, differing from the older ones somewhat seriously, though still accordant in the most essential features. His nomenclature of the seas and continents, derived from ancient geography, is certainly a great improvement on that of his predecessors, who had affixed to them the names of their friends and acquaintances among living astronomers. There has been some scepticism as to the reality of these "canals"; but in 1879 and 1881 they were all recovered by Schiaparelli, and several other observers, notably Burton, also made them out. Moreover, Terby finds, from drawings in his possession, that they had before been seen, though not understood or clearly recognised, by Dawes, Secchi, and other observers. At present the balance of evidence is certainly in their favour, especially as the observers at Nice report seeing them last spring. I do not think the same can be said in respect to another observation of Schiaparelli's on the same object, made in 1881. He then found nearly all of these canals—more than twenty of them—to be *double, i.e.* in place of a single canal there were two—parallel, and 200 or 300 miles apart. No one else so far has confirmed this "gemination" of the canals; but the planet does not come to a really favourable opposition again until 1890 and 1892, when probably the question can be settled.

The time of rotation has during the past year been determined with great accuracy by Bakhuyzen, who has corrected some errors of Kaiser and Proctor, and finds it 24h. 37m. 22·66s. In 1876 there still remained some question as to the amount by which the planet is flattened at the poles. The majority of observers had found a difference between equatorial and polar diameters amounting to between 1/100 and 1/30, while, on the other hand, a few of the best observers had found it insensible. The writer, in 1879, made a very careful determination, and found it 1/219, a quantity closely agreeing with the theoretical value deduced by Adams as probable from the motion of the newly-discovered satellites.

The Asteroids.—On May 1, 1876, the number of known asteroids was 163. To-day it stands at 258, 95 of these little bodies having been discovered within the decade, 45 of them by one man, Palisa, of Vienna, while our own Peters is responsible for 20.

None of the new ones are especially remarkable, *i.e.* some of the older ones are always more so; the most inclined and most eccentric orbits, the longest and the shortest periods, none of them belong to any of the late discoveries. One point is noteworthy, that the more recently discovered bodies are much smaller than the earlier ones. The first 25, discovered between May 1876 and October 1878, have an average opposition magnitude of 11·2, while the last 25, discovered since April 1883, average only 12·2; *i.e.* the first 25 average about 2½ times as bright as the last. Out of the whole 95, two are of the 9th magnitude (one of them, No. 234, was discovered as recently as August 1883), 14 are of the 10th, 33 of the 11th, 33 of the 12th, and 13 of the 13th. Of these last 13, 10 have been found within the past two years; and of the 12 others found in the same time, 6 are of the 11th magnitude, and 6 of the 12th.

It is clear that there can remain very few to be discovered as large as the 10th magnitude, but there may be an indefinite number of the smaller sizes.

The Major Planets.—As regards the planet Jupiter, the one interesting feature for the past ten years has been "the great red spot." This is an oval spot, some 30,000 miles in length, by 6000 or 7000 in width, which first attracted attention in 1878. At first, and for three years, it was very conspicuous, but in 1882 it became rather faint, though still remaining otherwise pretty much unchanged. In 1885 it was partly covered with a central whitish cloud, which threatened to obscure it entirely; but this season the veiling cloud has diminished, and the marking is again as plain as it was in 1882 or 1883. How long it will continue, no one can say; nor is there any general and authoritative agreement among astronomers as to its nature and cause.

In connection with observations upon this object, several new determinations have been made of the planet's rotation-period, and they all show that, as in the case of the sun, the equatorial markings complete the circuit more rapidly than in higher latitudes; a white spot near the equator gives 9h. 50m. 6s., as against 9h. 55m. 36s. for the red spot, which is approximately in latitude 30°.

We must not omit to mention Prof. Pickering's new photometric method of observing the eclipses of this planet's satellites. Instead of contenting himself with observing merely the moments of their disappearance and reappearance—an observation not susceptible of much accuracy—he makes a series of rapid comparisons between the brightness of the waning or waxing point of light during the two or three minutes of its change, using, as the standard, one of the neighbouring uneclipsed satellites. From these comparisons he determines the moment when the satellite under eclipse has just half its normal brightness, and this with a probable error hardly exceeding a single second, while the old-fashioned method gave results doubtful by not less than a quarter of a minute. Cornu and Obrecht have independently introduced the same method at Paris. When we have a complete twelve years' series of such observations, they will give an exceedingly precise determination of the time required by light to traverse the earth's orbit, and so, indirectly, of the solar parallax.

As regards Saturn, there is nothing to report so startling as Jupiter's red spot. A white spot, which appeared in 1877, enabled Hall to make a new determination of the rotation-period, which came out 10h. 14m. 14s. This is in substantial accord with an earlier determination of W. Herschel's (10h. 16m. 7s.), but involves a serious correction of the value 10h. 29m. 17s. given in most of the text-books. The error probably came from a servile copying of a slip of the pen made by some book-compiler, fifty years ago or more, in accidentally writing Herschel's value of the rotation of the inner ring, instead of that of the planet.

Much time has been spent in observations of the rings, and Trouvelot has reported a number of remarkable phenomena, most of which, however, he alone has seen as yet. The most recent micrometric measures have failed to confirm Struve's suspicion that the rings are contracting on the planet. Extensive series of observations have been made upon the satellites by H. Struve, Meyer, and others in Europe, and by Hall in this country. Hall's observations are especially valuable, and the series is now so nearly completed that we may soon hope to have most accurate tables. In the case of Hyperion, there is found a singular instance of a *retrograde* motion of the line of apsides of the orbit, produced by the action of an *outside* body, the effect being due to the near commensurability of the periods of Hyperion and Titan. This most peculiar and paradoxical disturbance first showed itself as an observed fact in Hall's observations; and, soon after, Newcomb gave the mathematical explanation and development. He finds the mass of Titan to be about 1/12,500 that of Saturn. It may be noted, too, that Hall's observations of the motions of Mimas and Enceladus indicate for the rings a mass less than 1/10 that deduced by Bessel: instead of being 1/100 as large as the planet, they cannot be more than 1/1000, and are probably less than 1/10,000.

The satellites of Uranus have also been assiduously observed at Washington, so that at present the Uranian system is probably as accurately determined as the Jovian, perhaps more so. The form of the planet has been shown to be decidedly elliptical (about 1/14) by observations of Schiaparelli and at Princeton; and the same observers have detected faint belts upon the disk, which have also been seen at Nice, and by the Henrys in Paris. Many of the observations appear to indicate a very paradoxical fact—that the belts, and consequently the planet's equator, are inclined to the orbits of the satellites at a considerable angle. The mathematical investigations of Tisserand appear to demonstrate that, in the case of a planet perceptibly flattened at the poles, satellites near enough to be free from much solar disturbance must revolve nearly in the plane of the equator; while those more remote, and disturbed more by the sun than by the protuberant equator of the planet, must revolve nearly in the plane of the planet's orbit. Thus the two satellites of Mars, the four satellites of Jupiter, and the seven inner satellites of Saturn, all move nearly in the equatorial plane, while our moon and Japetus move in ecliptical orbits. It is very difficult to believe that the satellites of Uranus, which are certainly not ecliptical and are very near the planet, do not move equatorially. And yet it is unquestionable that most of the observations with sufficiently powerful telescopes (my own among them) do seem to indicate pretty decidedly that the planet's equator is inclined as much as 15° or 20° to the orbit plane of the satellites.

As to Neptune, there is nothing new. One or two old observations of the planet have turned up in the revision of old star catalogues, and Hall, of Washington, has made a careful and accurate determination of the orbit of its one satellite, and of the planet's mass; while Maxwell Hall, of Jamaica, has deduced a very doubtful value of the planet's rotation from certain photometric observations of its brightness.

There has been some hope that a planet beyond Neptune might be found. Guided by certain slight indications of systematic disturbances in the motion of Neptune, Todd made an extended search for it in 1877-78, using the Washington telescope, and hoping to detect it by its disk, but without results. If such a planet exists, it is likely to appear as a star between the 11th and 13th magnitude, and may be picked up any time by the asteroid-hunters. But its slow motion, and the fact that our present charts give but few stars below the $11\frac{1}{2}$ magnitude, will render the recognition difficult.

The indications I have spoken of, and certain others first noted in 1880 by Prof. G. Forbes, and depending upon the behaviour of certain periodic comets, furnish pretty strong reasons for believing in its existence, though as yet they fall far short of making it certain.

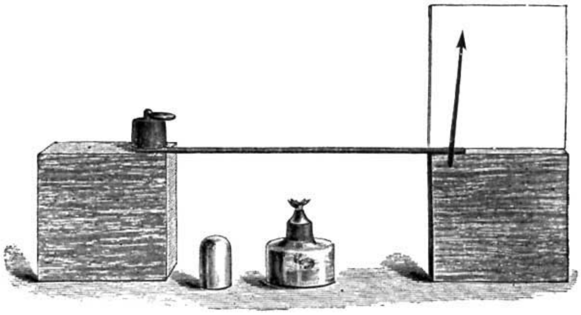
(*To be continued.*)

A LECTURE EXPERIMENT ON THE EXPANSION OF SOLIDS BY HEAT

I VENTURE to call attention to a simple and effective way of demonstrating the linear expansion of solids when heated, first suggested, I believe, by M. Kapoustine (*Journal de Physique*, December 1883, p. 576). It answers at least as well as the system of levers known as "Ferguson's pyrometer," which is usually employed for the purpose, while the cost of the apparatus is almost nothing, and any one can make it in ten minutes.

The principle is, to magnify the slight extension of a bar by causing the end of it to roll upon a needle, and thus turn the latter round and move a pointer attached to it through a sensible arc.

The figure given below will show the nature of the apparatus.



A small flat rod of the material to be examined, such as brass, iron, or glass, about 30 cm. long, 1 cm. broad, and 2 or 3 mm. thick, is laid upon two wooden blocks, placed about 25 cm. apart. A weight is put upon one end of the rod to keep it from moving; under the other end, at right angles to the length of the rod, is laid a fine sewing-needle, to the eye-end of which a light pointer of straw, about 16 or 20 cm. long, is attached by sealing-wax. Behind the pointer (which is painted black) a screen of white cardboard is fixed on the wooden block by drawing-pins.

When the rod is heated by a lamp-flame, the free end of it, as it expands, moves forward upon the needle and rolls it round, its movement being shown by the motion of the pointer. Even the slight expansion of a slip of glass is thus easily rendered evident to a class.

I have constructed for my own use a double apparatus on the same principle, in which the surfaces between which the needle rolls are of brass, ground true and flat. Two bars of different materials lie side by side, each having its own bit of needle and aluminium pointer, ranging over the same scale. They are heated equally by a broad flame (spirits of wine in a wide trough) and the difference of expansibility as well as the fact of expansion by heat is thus shown.

It is advisable to counterpoise the pointer by putting a shot or two into the lower end of the straw which projects below the needle, and cementing them in by sealing-wax. Also, before the experiment is shown to an audience, it is well to make sure that the needle rolls fairly and freely between the bar and the block. Such precautions, however, are not in the slightest degree necessary for school-work; for there is always one thing which gives the typical boy greater pleasure than to see an experiment succeed, and that is—to see it fail.

Eton College H. G. MADAN

COMPARATIVE STUDIES UPON THE GLACIATION OF NORTH AMERICA, GREAT BRITAIN, AND IRELAND[1]

OBSERVATIONS extending over several years upon glacial phenomena on both sides of the Atlantic had convinced the author of the essential identity of these phenomena; and the object of this paper was to show that the glacial deposits of Great Britain and Ireland, like those of America, may be interpreted most satisfactorily by considering them with reference to a series of great *terminal moraines*, which both define confluent

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[1] Abstract of a Paper read at the Birmingham meeting of the British Association, September 1886, by Prof. H. Carvill Lewis, M.A., F.G.S.